The Standard of Atomic Weights.

It is with considerable surprise, as a chemist, that I see in NATURE of April 22, p. 230, arguments as to the structure of atoms based on the deviations of the atomic weights of elements from whole numbers *on the standard* $O = 16{\cdot}00$ The reasons for the use of this arbitrary and inconvenient standard are now matters of ancient history, and the values of Stas, which were regarded as fundamental at the time when the standard was adopted, have now been shown by many independent lines of experiment to be inexact. It is almost pathetic to observe modern experimenters who have determined equivalents by the accurate analysis of hydrogen compounds, such as hydrogen chloride, methane, hydrogen bromide, ammonia, etc., all of which are more easily obtained in a state of purity, and analysed, than oxygen compounds, compelled to multiply their results by 1·008 in order to bring them into line with the standard of $O = 16{\cdot}00$.

A glance at the International Table of Atomic Weights will show that very few of the elements form oxygen compounds suitable for analysis, and the statement to the contrary, found in most elementary textbooks, is clearly inaccurate. A great number of equivalents, on the contrary, have been referred to $Ag = 107{\cdot}88$. This number can be brought into ratio with oxygen only through the intermediate link of nitrogen, the atomic weight of which has been most accurately determined by the analysis of ammonia. The latter involves the ratio 1·008 to get the ratio to $O = 16{\cdot}00$. But the atomic weight of chlorine has been most accurately determined directly to $H = 1{\cdot}00$, and the ratio $Ag : Cl$ is also very accurately known. From hydrogen to chlorine, from chlorine to silver, and from silver to a large number of other elements seems to be the most natural proceeding. Oxygen then comes in from the ratio $H : O$ found by Morley, Scott, and Burt and Edgar. This is now probably one of the most accurately known atomic weights. The above is one instance only of the extraordinary branch-chain methods now necessary in order to get the experimental numbers referred to oxygen.

On the theoretical side the advantages of the hydrogen standard are self-evident. No one has ever pretended that the adoption of oxygen as the unit has any theoretical significance; the retention of the number $O = 16{\cdot}00$ alone is sufficient to prove this. The accumulating evidence on the physical side, such as atomic numbers, the structure of atomic nuclei, the periodic law, and the like, all points unmistakably to the mass of the hydrogen atom as the natural standard. It is no longer correct to say, as is still done in elementary books, and even in other quarters, that the standard of atomic weights is a matter of indifference, and that, apart from experimental reasons, one element is as good as another. We have almost certain evidence that the hydrogen nucleus is a fundamental constituent of all atoms. Prout's hypothesis being thus reinstated, there can be no doubt as to the suitable standard of atomic weights, and Dalton's choice has had a most remarkable vindication.

When, therefore, arguments are advanced based on the standard $O = 16{\cdot}00$, it seems time to suggest that some steps should be taken to put an end to the prevailing confusion. Physicists have never taken kindly to the oxygen standard, and there is no longer any reason why chemists should be given needless trouble. I have, in my elementary lectures, made a practice of using the hydrogen standard, and thus avoiding all the confusion in connection with vapour densities, etc., which comes in with the other system.

There is one other point which seems to me of importance. On the oxygen scale the atomic weights of a number of elements differ by about half a unit from whole numbers. It has been conjectured that these elements are mixtures of isotopes, with atomic weights which are whole numbers. But if there is anything in the theory of isotopes to justify this, it can only rest on the hydrogen nucleus, and the atomic weight of hydrogen should be taken as unity. If this is done, it is found that the suspected elements are replaced by those not at present under any clouds of suspicion. The following table will illustrate this point:

Element	Atomic Weight $O = 16{\cdot}00$	Atomic Weight $H = 1{\cdot}00$
Chlorine	35·45	35·18
Magnesium ...	24·32	24·14
Silicon	28·3	28·1
Zinc	65·37	64·88
Copper	63·57	63·10

It may be that there is some real physical reason for taking $O = 16{\cdot}00$, and then supposing that, if some elements deviate from the whole number on this basis, they must be mixtures of isotopes, but this reason has so far escaped my attention.

There seems to me to be a good case for the Committee on Atomic Weights to consider whether the unit $O = 16{\cdot}00$, adopted largely on account of the persistence of Ostwald, is any longer necessary. At best it was a temporary decision, and all the reasons which were advanced in its favour have now lost their force. I am convinced that the arguments in favour of a return to Dalton's unit are so cogent that, once they are clearly realised, they will be admitted.

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April 23.

Mortlakes as a Cause of River-windings.

MORTLAKE on the Thames has a place-name which not only accords with the natural history of the place, but also supplies a word which might conveniently be brought into common use to signify a process which plays an important part in the development of every river system, just as the River Meander supplies a word to signify the windings of any river. The area between Barnes and the Thames was formerly an island *in* the river (Fig. 1), formed by a division of the stream into a northern and a southern arm reunited at the down-stream ends. The southern arm is now incomplete; the part of it remaining is included in the line of the Beverley Brook, which, having come from the south, turns to the east round a bold curve and joins the Thames. This leaves a gap between the convexity of the curve and the river at the point where it previously divided. This gap now forms the isthmus of a peninsula into which the island has been converted by the partial effacement of the southern arm of the divided river. Here Mortlake stands. It is on or near to the former line of the stream which has been in part effaced. This part has become a dead stream—a mortlake, the word "lake" having been used in the Middle English sense

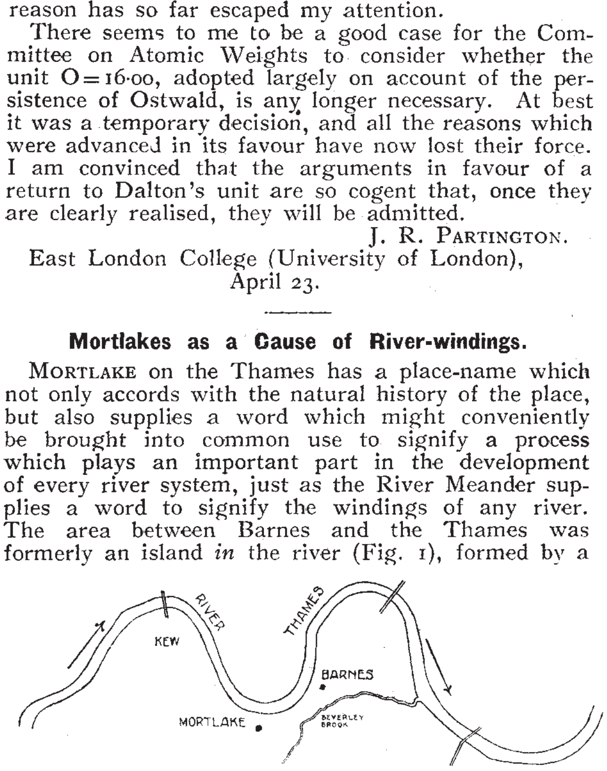

FIG. 1.

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as signifying a stream. Leland the Chronicler wrote that "there runneth a praty lake out of Sudeley Parke down by the Castle and runneth into Essebourne Brook at the south syde of Winchcombe." And again: "I passed over 2. or 3. small lakes betwixt Chiltinham and Gloucester and they resort to Severne." The word is still used in some places as meaning a stream; children on the Severn banks still sing of the moon as claiming to guide the ship "up the lake."

The first Lord Avebury, in writing of meanders ("Scenery of England"), mentions, as one of the results, "that the loop often remains as a dead river-channel or mortlake." Such loop-lakes are known in America as "oxbows." There is, however, a great difference. In the case of an "oxbow" the loop, formed by a lateral deviation of the river, has been entirely cut off from the main stream. A mortlake may be defined as the line of a closed part of one of the two sections of a river (previously divided so as to surround an island in the stream), the other channel remaining open and serving as a single channel for the river.

Of the two sections of a river enclosing an island, one of them at least must have a curved line. Two straight lines cannot enclose a space. If, then, the whole or a part of the stream in one of the sections be effaced, the remaining section, now forming the whole of the river, must have a more or less curved line. It must be a river-winding. The form and the length of it will, of course, depend on the shape and on the size of the former island. Thus a result of the formation of a mortlake may be one of those meanders or river-windings which are a familiar and picturesque feature in the landscape. The explanation of them has been a puzzle from classic times until the present. The subject was discussed at length by six contributors to Nature in November and December, 1907.

I have come to the conclusion that the conversion of river-islands into areas bounded by single streams, more or less curved, is part of the ordinary course of river development. This, in my view, may be briefly stated thus: A newly exposed part of the earth's surface receives the rainfall on every square inch of it, but the water will not flow away in the form of a sheet; minute runnels form, and these will not be in straight lines parallel to each other. I cannot imagine straight "primary consequent streams" as they are sometimes depicted. Even slight obstructions would turn them aside so that they would meet and coalesce, thus forming a miniature network of streams, each of the meshes enclosing an islet. Then the water on the up-stream side of every islet will have alternative routes before it. These routes may, for a time, be equally easy, but they will not continue to be so; one of them will be preferred, and this may not be the most direct. The stream which continues may be the one which meets with the least obstruction, or it may be the one most necessary for continuance as having to receive longer or more numerous tributaries. A channel no longer needed will cease to be used; it will silt up. Then the islet will become continuous with an adjoining islet. This process being many times repeated, islands of increasing size—which may be large and, possibly, of very great extent—will result. Ultimately, they will all cease to be islands, in the absence of need for two channels. The development, in my view, is not *from* "primary streams" *into* a "complicated network" of channels, but from the complexity of a network of channels towards the simplicity of one principal stream with tributaries converging towards it. Certain it is that in a river system as we see it there is no network.

I do not wish to suggest that the formation of mortlakes is the only cause of river windings. Thirty-seven years ago I pointed out (in a paper read before a local society, and printed at the time) the influence of tributaries as one cause; and I recognise others. But the same laws govern the development of all rivers. Although Herodotus found the rivers of Egypt to be different from other rivers, I, in imagination, see the life-history of the Nile as very strikingly depicted in its present course. I have elsewhere shown ("The Lower Severn," Proceedings of the Cotteswold Naturalists' Field Club, xvi., 1909) an outline picture of a thousand miles of the Nile below Khartum compared with one of ten miles of the Severn below Gloucester. The resemblance is so close as to lead to the remark that it almost seemed as if the one figure had been drawn in ink on the second page of a sheet of paper and the other by pressing the ink before it was dry against the opposite page. The size of the two rivers and the character of the rock formations being so very different, it is at least remarkable that the course of the two should be so very much alike. In the *Times* of a recent date (March 15) is a report from Dr. Chalmers Mitchell of his view from an aeroplane in passing above the railway between Wady Halfa and Abu Hamed. He saw "huge cliff-lines submerged at intervals by desert," which suggested the "proper bed" of the Nile. It is really a relic of the time when the area, now partly enclosed by the great sickle-shaped curve of the Nile, was a huge river-island two hundred miles wide and five hundred miles long. That which Dr. Chalmers Mitchell saw was the line of the eastern arm of the Nile; it is now the line of a long mortlake.

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Eiffel Tower Wireless Time-Signals.

It may interest a number of readers of Nature to know that the Eiffel Tower is at present sending out two additional sets of "scientific" time-signals. The scientific signals are arranged as a time-vernier, gaining about one beat in fifty. They have hitherto been sent at 11.30 p.m. G.M.T., followed at 11.45, after the ordinary time-signal is concluded, by numbers which give the moment of the first and the last signal of the set, according to the standard clock of the Observatory of Paris. A comparison can thus be made with the introduction of a very small error, often not exceeding one-fiftieth of a second. These valuable signals have suffered from two awkward features: In summer time they are inconveniently late, and the purring or snoring note (*ronflée*) on which they are sent is much obscured by atmospherics when the latter are bad, so that sometimes one failed to pick up the identification breaks which occur at the end of every sixty beats.

In addition to the old series, which remain unchanged, two new series are now being sent; these are on wave-length 2600 metres and a high musical note that cannot be confused with atmospherics. Otherwise they are the same as the original —300 dots, the 60th, 120th, 180th, and 240th being suppressed. They are sent: (1) at 10.30 a.m. G.M.T., the comparison numbers giving Paris time following after completion of the 10.45 ordinary signal; and (2) at 11 p.m. G.M.T., the comparison numbers being sent after the 11.45 ordinary signal, along with those which refer to the old 11.30 signal, the two references being distinguished by the letters ML (*musicale*) and RF (*ronflée*) respectively. The new series are beautifully clear, and ought to be of great service to those who require accurate time.

R. A. Sampson.

Royal Observatory, Edinburgh, April 17.

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